American Neurological Association.

FOURTH ANNUAL (ADJOURNED) SESSION.

(*Officially Reported by Dr. M. Josiah Roberts.*)

FIRST DAY'S PROCEEDINGS: Report of the Hammond Prize Committee.—Amendments to the Constitution.—Time of the Association's annual session.—New by-laws.—The Jewell and Hammond prizes for 1879.—Business before the Association.—Dr. Spitzka on the lower olivary body in man, the anthropoid apes, and other mammals.—Dr. Putnam's case of probable hemorrhage into the cervical enlargement.—Case presented by Dr. Spitzka.—Galvanometer, with remarks upon, by Dr. Putnam.—Dr. Webber's suggestions as to the nomenclature of the affections of the spinal cord.—Dr. Rockwell's remarks based on a case of loss of vision and recovery in brain disease.—Dr. Seguin on cervical paraplegia.—Dr. Beard's exhibit of apparatus for the relief of writer's cramp.—Dr. Putnam's apparatus for wrist-drop.

SECOND DAY'S PROCEEDINGS: Report of nominating committee.—The Association's publications.—Dr. Emerson's case of probable aneurismal tumors of the brain, and discussion.—Dr. Spitzka's demonstration of the brain of the menobranchus.—Dr. Webber on syphilitic disease of the cord, case, specimens and discussion.—Dr. Beard on how to experiment upon living human beings.—Dr. Seguin's case of, and remarks upon, amyotrophic lateral sclerosis.—Dr. Gray on cerebral thermometry, and discussion upon the same.—Dr. Kinnicut's case of generalized athetosis.—Dr. Gibney, history of a case of cervical paraplegia, discussion.

THIRD DAY'S PROCEEDINGS: Business.—Dr. Putnam on intra-ocular circulation, discussion.—Dr. Beard, cases and remarks.—Dr. Schmidt on the structure and function of the ganglionic bodies of the cerebro-spinal axis.—Dr. Spitzka on the signification of negative pathological evidence in insanity.

WEDNESDAY, JUNE 19TH.—AFTERNOON SESSION.

The American Neurological Association convened at the College of Physicians and Surgeons, in New York city, June 19th, 1878, and was called to order at 2:30 p. m.

Present at the session: Drs. Beard, Dalton, Edes, Emerson, Gibney, Gray, Hamilton, Hammond, Kinnicut, McBride, Mason, Putnam, Rockwell, Seguin, Spitzka, Van Bibber, and Webber.

In the absence of the President, Dr. J. S. Jewell, of Chicago, and the first Vice-President, Dr. F. T. Miles, of Baltimore, Dr. S. G. Webber, of Boston, became the president for this session.

As the minutes of the preceding session had been printed and distributed among the members of the Association, on motion, their reading was dispensed with.

REPORTS.

Dr. E. C. Seguin, of New York, in the absence of the chairman of the Hammond Prize Committee, reported that two essays were offered for the prize, on the anatomical and physiological effects of strychnia on the brain, spinal cord, and the nerves. The prize of $250 was awarded to the essay signed, "Nux vomica," and upon opening the sealed envelope which accompanied the essay, bearing the same signature, the author was found to be Dr. E. C. Spitzka, of New York.

The Secretary reported that the following amendments had been adopted: First, As proposed by Dr. Kinnicut, that the officers of the Association shall be a president, a vice-president, a secretary, who shall also perform the duties of treasurer, and two counselors. Second, As proposed by Dr. Emerson, to open the annual session of the Association on the third Wednesday in June. The vote for this amendment was as follows: Twelve for the third Wednesday, ten for the second Wednesday in June, and one for September.

The Secretary also reminded the Association that two new by-laws had been adopted at the preceding session, as follows:

Art. 7. The Council shall be empowered to take cognizance of violations of the constitution and by-laws by members; of persistent failure to pay annual dues; and of willful neglect to attend the meetings of the Association; and shall recommend to the Association proper action in the premises, such as sending a final notice, or dropping from the roll, as the Association may determine by a two-thirds vote of the members present.

Art. 8. That any member of the Association who shall be absent from any two consecutive annual sessions of the Association shall thereby cease to be a member thereof, but may be reinstated by a unanimous vote of the Council.

Dr. Seguin stated that he had been authorized by Dr. J. S. Jewell to offer a prize of $200 for the best original researches into the physiology of the nerves of the uterus and ovaries for next year.

Dr. William A. Hammond, of New York, stated that he would renew his prize of $250 for the ensuing year for the best essay upon a subject which he would hereafter mention.

Upon motion, the resignation of Dr. Walter Hay, of Dubuque, Iowa, was accepted.

THE COMMITTEE ON NOMINATIONS,

as appointed by President Webber, was as follows: Drs. Edes, of Boston; Shaw, of Brooklyn; Mason, Gibney, and Beard, of New York.

Dr. Hammond proposed the name of Dr. Frederick Bateman, of Norwich, England, as honorary member.

Dr. E. C. Seguin, Recording Secretary and Treasurer of the Association, made his report, which was accepted.

Upon motion, the President appointed Drs. McBride and Putnam as auditing committee to examine the Treasurer's accounts.

A motion was carried authorizing the Secretary to arrange for the reading of papers.

There being no further miscellaneous business before the Association, Dr. E. C. Spitzka proceeded to read his paper on

THE LOWER OLIVARY BODY IN MAN, THE ANTHROPOID APES, AND OTHER MAMMALS.

The paper substantially states that the first trace of this body found in the animal series was the homologue of the internal accessory olive of man, and, as such, reached a high degree of development in the parrot. It was continuous below with a part of the anterior grey cornu of the spinal cord, and this continuity was well observed in successive transverse, as well as in longitudinal sections of the human olive. In the placental carnivora this internal accessory olive sent out a v-shaped fold which, in its turn, was everted externally. The v-shaped fold was the homologue of the dentated olivary nucleus in man, while the everted portion constituted the rudiment of the external accessory olive. On tracing the development of this body through the bear, seal, catarrhine and platyrrhine monkeys, the complete homology of these parts could be demonstrated, and Duval was consequently wrong in stating that the carnivora only possessed the internal accessory olive.

The higher we pass in the animal scale, the more the v-shaped portion becomes convoluted, while the internal accessory olive becomes smaller. The olivary body of the chimpanzee is immensely superior to that of all other animals, and the differences between it and that of man are merely quantitative, and disappear when compared with the immense difference existing between the olive of the chimpanzee and of the baboon.

A slender bar of gelatinous nervous tissue is here seen to clearly connect the dentated nucleus with the internal accessory olive, and in some individuals of the human species this is equally clearly seen. There is considerable individual variation in the development of the human olive. In some the dentated nucleus is hardly more convoluted than in the chimpanzee; in others it is an extremely intricate body. Some interesting variations were observed in imbecile and insane subjects, particularly those suffering from hereditary or congenital insanities. Objections were made to Stilling's nomenclature, as adopted by Henle, but Meynert's views, except in so far as they referred to the seal, were indorsed.

Dr. Spitzka presented, in connection with the reading of his paper, a series of sections of olivary bodies arranged in order. Diagrams were exhibited showing the great similarity of the olivary bodies of the seal and bear.

REMARKS UPON DR. SPITZKA'S PAPER.

Dr. Putnam.—I would like to ask if there be any recent investigations which militate against or confirm Meynert's views relating to the connection of the olivary bodies with the posterior columns?

Dr. Spitzka.—There are none opposed to this view, but, on the contrary, every new discovery only confirms the opinion adopted from Deiters, by Meynert, that the restiform columns decussated through the olives, and the pathological experience that atrophy of one cerebellar hemisphere was apt to be associated with atrophy of the opposite olive, which is in perfect accord.

Dr. Webber.—I was in hopes to have finished the examination of a case in which there was atrophy of both lobes of the cerebellum, together with destruction of the transverse fibres of the pons, and to have presented the results to the Association, but have been unable to do so.

The second paper of the afternoon was by Dr. J. J. Putnam, of Boston.

A CASE OF PROBABLE HEMORRHAGE INTO THE CERVICAL ENLARGEMENT OF THE SPINAL CORD.

The patient, a female, more than thirty years of age, previously healthy, without constitutional disease, was seized one morning, after she had been menstruating a day or so, with a feeling of tightness about the chest which caused her to get quickly out of bed. She however immediately returned to her bed and was thereupon seized with violent vomiting and stertorous breathing, which lasted for some moments, followed by complete paralysis. Her temperature was elevated, pulse excessively rapid, pain in the back and shoulders, and paralysis essentially complete. This was the condition of the patient, when seen in consultation, two weeks after the attack. Two or three points were worthy of notice. She could move her arms and shoulders with comparative ease, but there was no motion of any account of the fingers or wrists of either side. Sensation was so affected that the application of a powerful electric current failed to excite pain. Dr. Putnam saw the patient some two months later. She had menstruated twice, and each time had attacks of vomiting as before. She could now both flex and extend the carpus and the hand, though motion in the fingers was entirely abolished.

Dr. Putnam thought it probable that the posterior white columns were intact, and there was little doubt but that the hemorrhage was in the cervical region of the spinal cord, its upper limit being the fourth or fifth cervical nerve. It was also probable that a post-mortem examination would not reveal the exact pathological condition, and that the distribution ot the paralysis was not to be explained upon anatomical grounds alone. The mode of occurrence of hemorrhage at the cessation of the menses, had as yet not been made clear, and its explanation was not simplified, when we take into consideration the fact, that very large quantities of blood can be forced into the circulation without raising the pressure of the blood in the carotids, so long as the vasomotor centre remains intact.

REMARKS ON DR. PUTNAM'S PAPER.

Dr. Beard.—I was interested in the Doctor's conclusions, because in the past few years I have observed, in certain conditions of the spinal cord where there was no inflammation, such as cases of spinal irritation, numbness of the ulnar nerve. Then, again, all of the fingers would get numb. Now, why is it that this nerve is affected to the exclusion of other nerves? I have not been able to answer, except there is a peculiar susceptibility of this nerve. I had a case last winter in whom exercise would produce this feeling.

Dr. Putnam.—I entirely concur with Dr. Beard, for I have observed quite a number of these cases of numbness in persons not subject to spinal irritation. In one case it occurred in a strong, healthy and well nourished man, also in several other individuals in good health, and again in some who were in bad health. In the cases that I have observed, the two middle fingers were affected, and occasionally the whole arm was involved, the attacks coming on after dark, or very early in the morning.

Dr. Beard.—Did you always examine the spine with great care?

Dr. Putnam.—I could not say positively that I did in the cases referred to, though I am in the habit of so doing.

Dr. Beard.—In my experience, spinal irritation occurs in four cases out of five.

Dr. Seguin.—As regards the pathology of the case described by Dr. Putnam, I am very glad that he used the word *probable*, for I am rather indisposed to admit, from suddenness of development, that there was hemorrhage, and I base this opinion upon what I think are facts. First, in infantile paralysis we have a sudden development of paralysis, and in twenty-eight cases there was only one case in which hemorrhage was found, and that was without a history. In a case of my own, observed when I was in the army in 1864, a man was brought into a hospital in this city, who had fallen down with complete paralysis below the neck. My diagnosis was hemorrhage into the spinal cord. Post-mortem examination showed error of judging from suddenness of palsy: the lesion was red softening, a condition which I believe is excessively rare. The specimen was exhibited in this lecture room by Prof. Clark. Besides, in hemorrhage into the spinal cord, we should have complete anæsthesia.

Dr. Putnam.—In regard to the feet, sensation was very much impaired.

Dr. Seguin.—Yet anæsthesia was not complete in your case.

Dr. Putnam.—I suppose that is a thing which is variable; a slight impulse is sufficient to give rise to impressions.

Dr. Seguin.—I admit that a small quantity of grey matter is sufficient to transmit sensation.

Dr. Putnam.—I would not hold with any tenacity to the pathology of the case.

There being no further discussion, Dr. Geo. M. Beard, of New York, proceeded to make some remarks upon

THE CAUSE OF THE TRANSMISSION OF ELECTRIC INFLUENCE ACROSS THE MIDDLE LINE OF THE BODY.

In Vol. II. of our transactions there was a short article by Dr. S. Weir Mitchell, of Philadelphia, entitled, "On the Transmission of Electric Influence Across the Median Line of the Body." The claim of Dr. Mitchell was, that when the electrodes were applied on the healthy side, the muscles on the diseased side would contract, the electric influence going across. The observation is correct; the only thing of interest is its causation and physical explanation. Dr. Mitchell thought it was a new fact in electricity, and so states in his paper. He referred to Prof. George F. Barker, Professor of Physics in University of Pennsylvania, who came to the conclusion that it was caused by induction; a mild induced current causing the contraction. It was very clear that it did not come from reflex action, because if the nerve trunk on the healthy side was excited by a strong galvanic current, no contraction was produced on the diseased side. The error was in supposing that there was induction. Prof. Barker's experiment consisted in taking a shallow glass dish, eight inches in diameter, into which was poured some salt water. On one side of the dish he placed the ends of two platinum wires, some inches apart, and connected them to a Thomson's galvanometer. On the opposite side of the dish he placed two copper wires, about six inches apart, these being connected with a single cell of the battery. When the battery wire was introduced, there would be a marked deflection of the galvanometer needle; when removed, there would still be deflection, but in the opposite direction. These observations are correct; I have many times made these observations during the past three years. Now, the question arises, what is it that causes a deflection of the galvanometer needle? Prof. Barker supposed, that because the galvanometer needle moved when the current was closed as the battery wire was placed in the dish, therefore it was an induced current. It is not an induced, but a diffused galvanic current. How far this diffusion extends, depends upon the nature of the conducting fluid. As I concluded from experiments made some years ago, this diffusion extends to great distances, two feet or more, so as to affect a galvanoscopic frog, or Thomson galvanometer. I made experiments illustrating the diffusion of electricity at the time that I was repeating the experiments of Hitzig and Ferrier on the brain, and also when experimenting with Edison on his alleged new force.

Now comes the point where Prof. Barker made his mistake: when you interrupt a current, you cause the diffusion to be more extended than when the current is continuous. And this is in brief the explanation of what has been observed. The observation of Dr. Mitchell is correct, but the explanation which he and Prof. Barker give, is incorrect. It is the ordinary diffused *galvanic*, and *not* any induced current that causes the contraction across the median line of the body, and the interest of the observation that it illustrates how very feeble galvanic currents may cause a paralyzed muscle to contract.

REMARKS UPON DR. BEARD'S PAPER.

Dr. Mason.—I did not read Dr. Mitchell's paper, but if I get, from Dr. Beard's remarks, a correct statement of the facts, I would say that Dr. Beard's explanation is perfectly satisfactory, and I am very much surprised that there could have been any doubt upon the subject, because it is so exceedingly elementary.

Dr. Seguin.—I had observed the reaction previous to the publishing of Dr. Mitchell's paper, and had not thought much of it, except that it was due to the transmission of the galvanic current which was being used at the time, and had not considered the matter worthy of investigation.

Dr. Seguin.—I have a case of double facial paralysis of

Bell's form, which developed within the course of two days, and though the patient had been paralyzed for ten days, on Saturday last there was an exceedingly well marked increase of excitability to both currents, in the nerve and muscles.

Dr. Putnam to Dr. Seguin.—Is there anything peculiar in the character of the reaction, in your case?

Dr. Seguin.—Nothing, sir.

There being no further remarks upon Dr. Beard's paper, Dr. Spitzka exhibited an interesting case, the physical signs of which were obscure. There was clonic spasm, involving every muscle supplied by the spinal accessory nerve. There had been considerable doubt as to the condition of the larynx. Upon making a careful examination, the left adductor cord was found to be paralyzed. There was a peculiar tone to his voice. The patient had been sick for six years. The longer he stands the worse the spasm becomes.

Dr. J. J. Putnam called the attention of the Association to the galvanometer of Dr. Mann, patented by Sprague, of Manchester. The peculiarity of this instrument was, that the divisions of the scale corresponded to units of quantity of electricity, instead of being merely arbitrary markings, as is usual. He spoke of the manifest advantage in the direction of accuracy by expressing the strength of current used in experiments and in therapeutics, in terms of the deviation of such a galvanometer as this, rather than by the number of cells, etc.

Dr. Seguin made an objection to the instrument, because there was no means of arresting the needle, and called attention to the fact, that after taking one observation with such an instrument, considerable time would necessarily elapse before a second observation could be made.

He thought that all scientific observers should use galvanometers of the same kind, and thus much confusion would be avoided. To bring about this, manufacturers should be induced to supply the trade with galvanometers whose units of registry were the same.

Dr. Putnam remarked that any galvanometer could have attached to it a calculated scale with divisions indicating units of electricity.

FIRST DAY—EVENING SESSION.

The Association was called to order at 9 p. m. by President Webber.

REPORT OF THE COUNCIL.

Dr. Seguin reported that the Council recommended that nominations of associate members be postponed.

The Council recommended for active membership Dr. L. C. Gray, of Brooklyn, N. Y., whose paper had been examined and approved. A vote by ballot resulted in his unanimous election.

The first paper of the evening was read by Dr. S. G. Webber, of Boston, and entitled

A SUGGESTION AS TO THE NOMENCLATURE OF INFLAMMATORY AFFECTIONS OF THE SPINAL CORD.

The names now in use were, as a rule, not expressive of the pathological changes which cause the symptoms. Fox, Leyden, Lange, and Erb, recognize only interstitial myelitis. In so-called sclerosis the connective tissue is primarily and chiefly affected, the nerve fibres secondarily. Owing to the different arrangement of the connective tissue in different parts of the cord, the pathological changes do not advance the same, and are not distributed alike in all regions of the cord. So-called locomotor ataxia is considered by Charcot a parenchymatous inflammation. The nerve fibres are primarily affected, and the interstitial tissue secondarily; it is a chronic parenchymatous myelitis. In acute myelitis, generally all tissues are so seriously affected that there is no possibility of drawing a distinction; yet cases of somewhat protracted duration are found where a distinction may be seen after death, at least. Two cases of acute interstitial myelitis previously reported were referred to, wherein the nerve fibres around the grey matter were not affected; but a short distance from the grey matter they underwent degeneration. The interstitial tissue had undergone little or no change.

The next paper was read by Dr. A. D. Rockwell, of New York, the title of which was

REMARKS BASED ON A CASE OF TOTAL BLINDNESS, WITH SUDDEN RECOVERY OF SIGHT, FOLLOWING BRAIN DISEASE.

The infrequency with which we meet with serious impairment of the function of nerves of special sense in cases of hemiplegia, following brain lesion, rendered the subject perhaps of secondary importance, but as every deviation from the common symptoms of any disease, materially aided in its comprehension, the Doctor proposed to glance very briefly at what he considered the more interesting features connected with this topic. His remarks were suggested by a single case in which sight was suddenly lost and as suddenly regained. The fact that complete unilateral paralysis never follows cerebral lesion, has been thought to disprove the suggestion generally accepted, that the will in calling into activity the muscles of the opposite side, acts through the corpus striatum; but the hypothesis of the connection of spinal nuclei by commissural fibres that has been advanced to harmonize this suggestion with the fact·that bilaterally acting muscles are exempt from paralysis following lesions in and around the corpus striatum, is ingenious and plausible.

When we come to the consideration of sensory phenomena, equally interesting facts are observed. As a matter of fact we know that not only do ordinary lesions of one hemisphere fail to impair sight, but that almost complete destruction of one hemisphere may leave these functions intact, through the power of the opposite hemisphere to take cognizance of visual impressions. There are probably no exceptions to this exemption in damage to the hemispheres, as regards hearing, since the nuclei of the two auditory nerves lie near the junction of the pons with the medulla.

The determining causes of hemiplegia, which at the same time may occasion defects or complete loss of sight, are, first, where the superior peduncles which are closely related to the visual centres, suffer damage either by hemorrhage or occlusion of vessels. Second, when with a thrombosis in the carotid and middle cerebral arteries of one side, the obstruction is prolonged into the ophthalmic artery. Third, when one of the posterior cerebral arteries is obstructed by a thrombosis. Fourth, the optic tract may be compressed by hemorrhage into the substance of the crus cerebri or lower part of the sphenoidal lobe.

The case which the Doctor reported was not recent, but came under his notice in the autumn of 1866. The patient, a middle aged gentleman, had some years previous suffered from a convulsive seizure which was supposed to be epileptic in character, and this at long intervals had been repeated. Two months before he came under Dr. Rockwell's notice he had an epileptiform attack resulting in incomplete paralysis of one side of the body and total blindness. For two months after the attack, while the arm had almost wholly regained its normal power, the strength of the leg was still impaired to a considerable degree.

A powerful induction current was applied simultaneously to both eyes of the patient for about two minutes, resulting in an immediate and permanent restoration of sight. Judging from the reports of other cases and the probability of the purely functional or indirect cause of the blindness, recovery would probably have taken place spontaneously, and was only hastened by the reflex stimulation of the electricity.

It is notorious that the most profound derangements of the functional activity of the nerve centres may exist, and yet by none of our advanced methods of investigation has it been possible to discover any appreciable lesion; and until we have more accurate knowledge in this direction, the theory of reflex influence upon which we must fall back for explanation, although beyond the sphere of absolute verification, is at all events plausible, and to a certain extent, rational.

REMARKS UPON DR. ROCKWELL'S PAPER.

Dr. Putnam.—It is certain that loss of sensibility may be restored, although there is an organic lesion which is undoubtedly the cause of it, and as blindness is tolerably easily obtained in animals by injury to the hemispheres, it would seem to me not out of the question, that it was a lesion in the brain.

Dr. Webber.—Would not the complete recovery which took place exclude embolism in the retina of the eye?

Dr. Putnam.—Was hemiplegia permanent?

Dr. Rockwell.—It was so far as I know.

Dr. Spitzka.—I do not believe that any cranial nerve is subject to so much variation as the optic. There are several cases on record where there was no decussation at all. One of these cases is indorsed by the great anatomist Vesalius.

Dr. Seguin.—Was there blindness in both eyes?

Dr. Rockwell.—Yes sir, blindness existed in both eyes.

The next paper was read by Dr. E. C. Seguin, of New York, on

CERVICAL PARAPLEGIA.

Three cases were related, all presenting degrees of atrophic paralysis in both hands and arms, with more or less anæsthesia. In one case there was paralysis of the ciliary sympathetic fibres in one eye, on the same side as the greater wasting. In Case I., there were symptoms indicating progressive disease, such as a broad constriction feeling about the thorax and great weakness in the legs. This case was treated with the satisfactory result of relieving the last named symptoms and limiting the disease in the hands. The patient was seen a few months ago (four years after treatment) and no progress had been made by the affection. Case II., of moderate severity, and probably syphilitic, was entirely cured after several months' treatment. Case III. has been under observation only a few weeks.

REMARKS UPON DR. SEGUIN'S PAPER.

Dr. Putnam.—I would doubt the therapeutic value of the paper in reference to the first case, for I had a case similar to it in which there was great improvement, although there was almost no treatment. There were symptoms of hemiplegia. Recovery was complete, except from the atrophy of the muscles of the arm, in which there was no improvement.

Dr. Seguin.—There was no improvement in the hands in my Case I., but the relief from apparently impending paraplegia was very striking.

Dr. Beard.—Was there any attempt to differentiate the effect of the remedies used?

Dr. Seguin.—Not in the first case, for the records were not very scientifically made.

Dr. Putnam.—The parts which are not very severely affected are likely to recover, and might this not be fairly expected in Dr. Seguin's case, that it would pass away? I would not run the risk of no treatment, though I have great faith in spontaneous recovery.

Dr. Spitzka.—Do you think syphilitic cases will spontaneously get well?

Dr. Seguin.—I should rather doubt it. I should like to hear an expression of opinion as to the frequency of these cases.

Dr. Webber.—I cannot recollect any cases where the paralysis ceased, but I have had cases where the paralysis began and the patient died, but have had no autopsies. One in which the trophic part of the spinal cord was not affected, there was no diminution except in one muscle of the leg, and that was doubtful.

There being no further remarks upon Dr. Seguin's paper, Dr. Geo. M. Beard exhibited a simple apparatus, devised by one of his patients, for the relief of writer's cramp. The gentleman who invented it could not write his own name, but now, after the use of this instrument and electricity, was able to write ten hours per day. The great factor in the production of writer's cramp is the pressure upon the pen-holder, but in this instrument the evil referred to is wholly obviated.

Dr. Putnam showed a new apparatus, resembling "Barwell's Elastic Muscle," for giving support to the hand in cases of wrist-drop.

THURSDAY, JUNE 20TH—AFTERNOON SESSION.

Meeting was called to order by President Webber, at 2.30 p. m.

Dr. Edes read the report of the nominating committee recommending Dr. F. T. Miles, of Baltimore, for President; Dr. S. G. Webber, of Boston, for Vice-President; Dr. E. C. Seguin, as Recording Secretary and Treasurer; Dr. J. J. Mason, and Dr. F. P. Kinnicut, of New York, as Council. Upon motion, the report was accepted.

A check for $250 was received from Dr. Hammond and handed over to the successful competitor for his prize, Dr. Spitzka.

Upon motion of Dr. Shaw, the Council was requested to confer with Dr. Jewell, and determine the conditions upon which the Jewell prize would be awarded.

Dr. Putnam referred to the publication of the papers of the Association, and called attention to several objections to the method now adopted. He urged that some means be determined upon whereby a cheaper publication and a wider circulation of the Association's papers could be insured. After some discussion it was decided, upon motion of Dr. Hammond, to refer the whole matter to the Council, with power to take final action. By permission of the Council, members of the Association can publish their papers where they choose.

Dr. McBride made the report of the auditing committee, which was accepted.

The next business of the Association was the reading of papers, the first being by Dr. N. B. Emerson, of New York.

A CASE OF PROBABLE ANEURISMAL TUMORS OF THE BRAIN, WITH SPECIMENS AND DRAWINGS.

Dr. Emerson presented specimens and drawings of small tumors from the brain of a man who died in the Hoboken Church Hospital. The patient, aged 46 years, seaman, complained of a racking pain located in the forehead and occiput, violent exacerbation at night, with delirious attacks; memory was greatly impaired, so that a history was not obtained. His wife stated, at the time of his death, that he had suffered from convulsive attacks for twelve months. None were observed while the patient was in the hospital until the very last. He several times urinated in bed while asleep. His walk was feeble, but there was no paralysis, and sensibility was unchanged. While in hospital, subcutaneous hemorrhages appeared over face and neck, of irregular shape and size. Mucous membranes were free from hemorrhage and pale, so far as visible. Sight extremely poor, no measurements; ophthalmoscopic examination showed atrophy of the nerve, and retinal hemorrhages.

Death occurred suddenly during a delirious attack, and was immediately preceded by convulsions. Autopsy revealed pachymeningitis across the track of each middle meningeal artery, and chronic inflammation of the meninges, especially at the base of the brain. After hardening in potassium bichromate, the small tumors above mentioned were found more frequently in the cortical portion of the cerebrum and attached to the meninges at the base of the brain. In order to discover the locality of these little bodies, Dr. Emerson probed the brain in every direction with a fine sharp-pointed needle. The moment one of them was touched, its presence was indicated by the resistance offered. By this method, which he demonstrated before the Association on the brain in question, it is possible to discover a deep-seated tumor of hard consistency without in the least damaging the brain itself. The diameter of the tumors varied from 2.5 to 4.5 mm., and were enveloped in a capsule of proliferating connective tissue. Sections of tumors, stained and mounted, showed centrally, amorphous matter and fat granules; next, a zone of granular matter with traces of fibrils, also leucocytes; next, a zone of proliferating tissue with large numbers of nuclei or leucocytes in groups at the periphery.

Dr. Emerson was at first inclined to look upon these tumors as gummata, but at length came to look upon them as old healed aneurisms.

Dr. Emerson expressed his obligations to Dr. Wm. R. Fisher, of Hoboken, and Dr. O. D. Carpenter, of Jersey City, to whom he was indebted for the privilege of using the case.

REMARKS UPON DR. EMERSON'S PAPER.

Dr. Spitzka.—About a year and a half ago, I examined some gummatous tumors, not displacing the brain substance, but replacing it. While most of them looked similar to the tumors we have just seen, there were some that extended through the two first layers of the cortex beyond the blood-vessels, which I preferred to consider as miliary myxoma of a syphilitic nature. There was one of these near the origin of the third nerve.

Dr. Seguin.—I would state that I am disposed to look upon the tumors which Dr. Emerson has presented, as aneurismal, for the following reasons: The central portion of the tumors looks like thrombus undergoing fatty degeneration, the capsule resembles changed and thickened arteries which I have in my possession. I fail to see in these specimens any of the young cell formation which is so characteristic of gummata; also the absence of continuity between the tumors and the normal brain tissue; their size is, however, rather greater than that of ordinary miliary aneurisms.

In this connection, perhaps, I may be permitted to show some miliary aneurisms prepared by Dr. Amidon, of the New York Hospital. They show very beautifully the peri-arterial changes, and the fatty degeneration of the muscular coat. Dr. Amidon has devised a method of obtaining these aneurisms, which is simpler and much more rapid than Charcot's; he disintegrates the nervous tissue by a stream of hot water.

Dr. Spitzka.—As no gummata are known to occur centrally, I have always thought they proceeded from the meninges. I can exclude aneurism in my case, for I have found all the evidences of syphilis.

Dr. Emerson.—In looking at one of these specimens I found along its edge what appeared to be little extravasations, but I did not find any in the brain. The heart, in this case, was opened, but I could not say that it was examined.

Dr. Edes.—I have been struck with the resemblance of this drawing to some specimens of endarteritis which I have, without the same contents. The contents would, of course, be referred to the clot. I must say that I am in favor of aneurism, for it has been shown that endarteritis, occurring in a syphilitic patient, is not necessarily syphilitic, because other things give rise to it.

Dr. Spitzka.—In the case referred to by Dr. Edes, was there disease of the adventitia?

Dr. Edes.—I could not say as to that, positively.

Dr. Edes.—I have at home a very pretty specimen of aneurism, taken from the pons, which in some respects resembles the tumor here upon the slide, and also resembles somewhat that which is mounted in wax. The brain from which I took the specimen contained several. The parts around the aneurism were stained so as to be easily recognized by the naked eye alone, the thick walls of the vessel could be seen, and at the dilatation, where the blood had broken through, the thinness of the walls of the perivascular sheath of the adventitia was easily recognized. The size of the tumor was as large as those which Dr. Emerson has presented, or larger.

DEMONSTRATION OF THE BRAIN AND CORD OF MENOBRANCHUS.

Dr. E. C. Spitzka exhibited specimens of the spinal cord, medulla oblongata, and hemispheres of the menobranchus, which showed the immense size of the nerve-cell nuclei, and the peculiar arrangement of the fibrils constituting the axis cylinder, and other processes around the latter, giving the appearance as if a nucleus had been fortuitously entrapped in their meshes. The gradual transition of the epithelium of the central canal to nerve cells was clear and distinct. Of the distal nerve cells, one had a sharply-marked axis cylinder process extending to the periphery of the cord at the point of egress of the anterior roots. The brain cortex showed an embryonic structure, and the relations of the epithelium of the choroid plexus and tela choroidea were unusually distinct and interesting.

REMARKS UPON DR. SPITZKA'S PAPER.

Dr. Edes to Dr. Spitzka.—Have you been able to trace the processes from one cell to another cell?

Dr. Spitzka.—In one case, in the human spinal cord, I think I have unquestionably traced an anastomosis. Also in the sea-turtle I think I have an appearance of the same kind.

Dr. Seguin.—While studying in Bonn I found what I supposed to be an anastomosis, but upon looking at it with a one-fifteenth inch immersion lens I found the appearance due to superposition of the processes.

Dr. Putnam.—What is the character of the nerve tissue cells?

Dr. Spitzka.—From their position I consider the thick congregation of cells as the germ cells of higher animals, and the scattered cells as connective tissue cells. As everything in the menobranchus is embryonic, you cannot say which is which.

Dr. Seguin.—I would like to ask Dr. Mason if, in his researches upon the spinal cord of the alligator, he has found anything peculiar?

Dr. Mason.—As yet I have not had an opportunity of studying the sections which I have prepared.

Dr. Seguin called attention to the history of an anomalous case of sclerosis. The patient had as symptoms persistent numbness in hands, forearms, and legs. No other symptoms except on two occasions, an attack of fulminating pain. Upon post-mortem examination he found sclerosis in the posterior columns, chiefly in the columns of Goll, but alsoinvolving a part of the radicular zones. Sections from different points in the spinal cord were exhibited.

The next paper was read by Dr. S. G. Webber on

A CASE OF PROBABLE SYPHILITIC DISEASE OF THE SPINAL CORD, WITH SPECIMENS.

The case was interesting on account of the rare pathological changes. The cord was sent to Dr. Webber from the country, and there was no notice of changes elsewhere. Patient had syphilitic history, but no secondary symptoms; no venereal excess; no injury to back or head; lived in good quarters; four or five weeks before death he experienced numbness in the side of his nose, face, and left thumb; about the same time had pains in back, and both arms and legs, which became so severe that in two weeks he was obliged to take to his bed; had retention of urine, constipation of the bowels, no disturbance of eye-sight, but ptosis on the right side; eyes, pupils, and facial muscles readily responded to the will; motion of arms, hands, and legs possible; sensation in legs and hands rather diminished; no prickling sensation.

The greatest change in the cord was found just below the cervical enlargement. The membranes opposite the lateral columns were thickened and adherent; central grey substance was deformed; centre of the cord was occupied by a mass of diseased tissue, in which were seen numerous vessels with extravasated blood pigment and blood corpuscles, many nuclei and much fibrous tissue; also many enlarged spider cells, which seemed to lie near the root of the anterior cornua. Below this a section showed the most changes in the right lateral columns, the morbid changes being similar, except an increase in the number of spider cells.

Above first section in cervical enlargement the chief changes were, increased number of enlarged spider cells in the anterior cornua, and degeneration of the posterior columns. Posterior columns were also affected in the dorsal region below the part most diseased. Hence, changes in the posterior columns may have been an independent lesion. The change consisted of an enlargement and increase of the spider cells; the neuroglia fibres were increased, interlaced and wavy; nuclei were increased, the nerve fibres fewer, and the walls of vessels thickened.

The case is of interest for the enormous increase of spider cells. The development of new-formed, tissue was at the expense of the nervous fibres and cells, which seemed to have been pushed aside or supplanted. The new-formed tissues may be considered to form a tumor composed of the normal interstitial tissue; that is, it is a variety of glioma.

Dr. Webber exhibited several sections of the cord of the above case.

REMARKS UPON DR. WEBBER'S CASE.

Dr. Seguin.—I would like to ask if Dr. Webber has any explanation of the face symptoms in his case?

Dr. Webber.—The specimen was sent to me, and there was no history of a lesion elsewhere.

Dr. Seguin.—The question of spider cells is one which is attracting considerable attention at the present time. Dr. Shaw and I have made some observations in this direction. He found them in making examination of specimens of central myelitis and disseminated sclerosis, and I have demonstrated them in the lateral columns in amyotrophic lateral sclerosis and in sclerosed posterior columns. The question in Dr. Shaw's mind and my own, is, how far can we look upon these cells as evidence of abnormal cellular nutrition in contradistinction to nervous degeneration?

Dr. Webber.—In the drawing from the case of sclerosis, one of the spider cells is seen connecting with the artery or at least with a blood-vessel. I have found that quite a common connection, the processes of spider cells running into the walls of the artery and connecting with a sort of infundibulum. In one case I saw one of the processes of a spider cell take a circuitous course to the artery, while another took a direct course to the same artery.

Dr. Seguin.—I should like, Mr. President, your views as to whether spider cells are to be relied upon as evidences of inflammation.

Dr. Webber.—They are part of the normal constituents of the cord, and can be seen in almost any section, but of course the nerve fibres obscure their demonstration. A spider cell growing in a group of nerve fibres, which are transversely cut across, very soon loses its processes in the septa separating the nerve fibres one from the other, and it is not always easy to make out the cells from which these processes arise, these cells having nuclei and sometimes nucleoli, and sometimes I have seen two or three little fibres radiating from a nucleus. Being the normal constituents of the cord, like all other constituents of the neuroglia, when an inflammation is set up these spider cells enlarge, and perhaps imbibe carmine more readily, so that the processes given off by them are more readily seen. Sometimes these spider cells and their processes become very strongly tinted with carmine. After the inflammatory process has gone on for a length of time and has become quite chronic, a change takes place in these spider cells, and instead of being more deeply tinted by carmine, they become less strongly stained and it is sometimes difficult to see them. They then become reduced many times, merely to a nucleus that is pretty large. Around this nucleus, very early, can be seen the outline of the cell, which, having taken no carmine whatever, is translucent and without color. This I have looked upon as the stage of degeneration or atrophy of these spider cells. The interstitial tissue becomes condensed and solidified in the last stage of the disease. In the drawing which I presented, I think there was one of these spider cells which had passed through this degenerative process.

I have examined for nuclei very carefully in cases of so-called sclerosis, and have found that it was possible, in a very large majority of cases, to trace the outline of the spider cell.

So I think it is fair to conclude, that the nuclei of these cells are merely the nuclei of the spider cells, and that, owing to non-coloration by carmine or other causes, we cannot trace the outline of the spider cells. Taking this view of it, the above described changes are merely changes in the normal structures of the cord, and may be considered inflammatory.

Dr. Seguin.—Dr. Webber's views are in accord with my own, but I wish to call attention to the cloudy appearance of these cells when they become enormously enlarged and the number of processes seem to be larger than normal.

Dr. Webber.—This appearance can be seen in one of the specimens now under the microscope. The cell is in rather a thick portion of the cord. I would add, that it is my opinion, that these cells sometimes undergo fatty degeneration and are absorbed.

Dr. Shaw.—I think that evidence of the last statement which Dr. Webber made may be seen in disseminated sclerosis. I think these cells do undergo degeneration. It has appeared to me, that where specimens were kept in alcohol, the cells could not be seen.

Dr. Webber.—I have been unable to satisfactorily observe specimens freshly placed in Canada balsam. It is only after the lapse of a considerable length of time and the specimen has become thoroughly permeated with the mounting fluid, that study of these cells is followed by satisfactory results.

There was another point to which I neglected to call attention. Spider cells seem to undergo a great many changes. In a swollen condition the cells seem to be filled with some material. In such, I have seen groups of from two to six nuclei, as if white blood corpuscles had migrated into the cells, and accumulated there. I found one cell in which there were five or six nuclei grouped together in one end, but it is very common to find cells with two large nuclei, the rest of the cell having the appearance as though it had been filled with a fluid which had become solidified through the agency of the reagents.

Dr. Shaw.—I would like to ask Dr. Webber, if, in those cells where there were a number of nuclei, it was not due to division of the original nucleus? In drawings from my case you will see a contraction in the nucleus as if it was about to divide.

Dr. Webber.—Yes, it is quite possible that is the case.

Next followed the reading of a paper by Geo. M. Beard, M. D., of New York.

HOW TO EXPERIMENT ON LIVING HUMAN BEINGS.

Mr. John Stuart Mill, in the preface to his work on, "Logic and the Principles of Evidence" observed, that, while we may instinctively avoid errors in the search for truth, yet it is an advantage not to be dispensed with, to have a rational understanding of the philosophy of reasoning, so that we shall not be forced to depend on dumb instinct alone.

In experimenting with living human beings there are six sources of error which physiologists and physicians guard against instinctively, but which ought to be and can be reduced to a positive statement. These elements of error come from the nervous system, their study therefore belongs to neurology. It is because of the backwardness of neurology that this subject has thus far been passed by. Dr. Beard had already called attention to these six sources of error in experimenting upon living human beings, very briefly, in an article on the physiology of mind reading. But in his remarks before the Association he proposed to discuss the subject in greater detail. He found in making experiments upon living human beings for the purpose of studying the physiology of mind reading, and also in making experiments relating to the action of mind on body, that this whole matter was as unexplored as the north pole, and that it was necessary to hew one's way through infinite obstructions at every step. The elements of error in experimenting with animate objects were indicated by Bacon, under the strange headings, "Idols of the tribe, of the den, of the forum, of the theatre," and later writers upon evidence and principles of science have repeated or assumed the Baconian formulas, but the special elements of error in experimenting with *living* human beings, have thus far escaped conscious exhaustive analysis. The six sources of error are as follows:

First Source of Error.—The phenomena of the involuntary life in both the experimenter and the subject experimented on.

Under this head are embraced trance, with all its symptoms and all the interactions of mind and body that are below the plane of volition, or of consciousness, or of both. Without a knowledge of this side of physiology, scientific experimenting with living human beings is impossible.

Second Source of Error.—Unconscious deception on the part of the subject experimented on. This element of error slips into all the ordinary experiments with new remedies and supposed new forces in the animal body, thus corrupting science at its very sources. The neglect of this element of error, even although all the other errors were guarded against, would vitiate the utility of such experiments as those of the committees of the French Academy on clairvoyance and mesmerism. In regard to guarding against these two sources of error, two things are necessary for the experimenter: First, a thorough knowledge of the involuntary life; second, *the subject experimented on must always be deceived.* The three methods of deceiving the subject, by which alone the element of error from mind acting upon body can be eliminated, are these: First, by doing something when the subject experimented on believes we are doing nothing. Second, by doing nothing when the subject believes we are doing something. Third, by doing something different from what the subject believes is being done.

Third Source of Error.—Intentional deception on the part of the subject. The methods of deception already described suffice to guard against all deception, intentional or unintentional.

Fourth Source of Error.—Unintentional collusion of third parties. By third parties is meant witnesses, audience, bystanders, seen or unseen. The best illustration of error from this source, is the aid which audiences give to a performer, by their silence when the mind reader wanders from the object for which he is searching, and by their murmur or applause when he nears or reaches it. This is quite analogous to the cry, "hot!" or "cold!" in the game of "blind man's buff." Dr. Beard, in his experiments in mind reading, found it necessary to send out all witnesses, or insist upon their being perfectly silent or motionless.

Fifth Source of Error.—Intentional collusion of third parties. Under this head come the aid that assistants give in experiments like those of Houdin and Heller.

Sixth Source of Error.—Chance and coincidence. The experiments during the past year of Charcot with metalloscopy and metal therapeutics, have given special interest to this subject. Admitting all the facts which Charcot claims, considered as facts alone, it is clear from his own statement that he did not consider or allow for these six sources of error. He did not prove whether the results obtained by putting metals on the bodies of hysterical patients, were subjective or objective, but in his lecture on metal therapeutics assumed that the question was settled and that the results were objective. His experiments were, therefore, of no value scientifically. If it should be proved hereafter by Charcot himself, or by others, that the application of metals, gold, silver, copper, have a real objective effect with regard to the mind of the patient, it is none the less true that he has not thus far proved it, for results far more remarkable Dr. Beard stated he had obtained in experiments with living human beings, by the mind acting upon the body alone. If it should be proved hereafter that these results are objective, another series of experiments would be needed to determine whether they were the result of pressure or of absorption of feeble currents of electricity generated by the metals.

From the reduction of this subject to a science, two results must follow: First, there will be more precision in the results of our experiments with new remedies and in modes of treatment. Second, men of ordinary ability can succeed in experimenting with living human beings where now men of great genius fail. With these six sources of error before them and fully understood, any medical student could make the experiments of Charcot, and settle for all time the question at issue.

REMARKS UPON DR. BEARD'S PAPER.

Dr. Mason.—It has occurred to me, that to make Dr. Beard's paper strong and precise, the points should be given in which Dr. Charcot has erred. I would like to know if Dr. Charcot has ever claimed curative effect in these metals.

Dr. Kinnicut.—Dr. Charcot expresses himself as a supporter of that view. In his lectures delivered last autumn and published, he is reported to believe in the efficacy of these metals, as shown by cases in his own hospital, but he gives no theory so far as I know, except to claim that it is not due to expectation and attention. If I remember rightly, all of his cases were not hospital cases. One occurred in private practice, and he states that all possible sources of error had been removed.

Dr. Seguin.—I think Dr. Beard's paper is very interesting and a valuable contribution. I think it will help us in our very blind and groping way of testing medicines. I hope to see a reform obtained by bearing in mind these sources of error, yet in private practice it is becoming more and more difficult, because our patients are becoming more and more clamorous to know what medicine is being used, and what is expected of it.

Dr. Putnam.—I saw the experiments of Dr. Charcot performed, and though I was at that time unable to discover any source of error, yet I am strongly inclined to agree with Dr. Beard, and I must say the impression made upon me by Dr. Charcot was, that he was wanting in the necessary qualities to successfully perform experiments of that kind.

The next paper was by Dr. E. C. Seguin, entitled

AMYOTROPHIC LATERAL SCLEROSIS. SPECIMENS.

The patient had presented, in the course of a two years' illness, all the classical symptoms of the disease, viz.: atrophic paralysis of the hands and forearms, contracture of all the limbs, signs of labio-glosso-pharyngeal paralysis. The postmortem examination showed no lesion visible to the naked eye, except smallness of some anterior rootlets; but the study of numerous microscopic sections of the spinal cord and medulla oblongata showed the following lesions: First, a focus of disease in the cervical enlargement, consisting in atrophy of the ganglion cells of the anterior horns, especially in the lower part of the enlargement, and sclerosis in the postero-lateral column. Second. Sclerosis of the postero-lateral columns throughout the rest of the spinal cord above and below the cervical enlargement. Third. Destruction of nearly all the ganglion cells in the nuclei of the hypoglossal nerves. The duties of Secretary to the Association had prevented the completion of the study of the sections, so that many points required elucidation. Dr. Seguin was, however, inclined to agree with Prof. Flechsig in locating the sclerosis found in this disease in the coarser pyramidal columns, and not, as Charcot expresses it, in the lateral columns.

Dr. Webber.—I would like to ask whether the lesion of the white columns in these cases affected the interstitial or nerve fibres first?

Dr. Seguin.—Probably, the interstitial tissue. I was very desirous, and am glad to have had the significance of enlarged spider cells explained, before I read my paper, for that very reason. I found, and can demonstrate in these specimens, spider cells in the sclerosed columns.

There being no further discussion the Association adjourned to meet again in the evening.

SECOND DAY—EVENING SESSION.

The Association was called to order by President Webber at 9:30 p. m., and there being no business to transact, Dr. Landon Carter Gray, of Brooklyn, proceeded at once to read his paper on

CEREBRAL THERMOMETRY.

The paper contained the results of an investigation of the cerebral temperatures of one hundred and two individuals. The same points for placing the thermometers were adopted as in the researches of M. Paul Broca, which were reported in Aug., 1877, to l'Association Francaise pour l'Avancement des Sciences, but which only embraced twelve persons. To these points Dr. Gray has given the name of stations. They are situated as follows: One on each side somewhat back and above the commencement of the external angular process of the frontal bone, being the frontal stations; one on each side just above the ear, being the parietal stations; and one on each side of the occiput, being the occipital stations. In addition, Dr. Gray ascertained, by means of certain measurements which are detailed in his paper, the distance of the fissure of Rolando at its vertical termination from the furrow formed by the junction of the nasal bones with the nasal process of the frontal. This distance was, on the average, 6⅞ inches. Around the fissure of Rolando are the four vertical stations. Seguin's surface thermometers, made especially with a view to obviating all error from any accidental pressure upon the bulb, were employed; and they were held in position by a head-gear made of ribbon or soft morocco. The conclusions arrived at by Dr. Gray were:

"1. The average temperature of the left frontal station is 94° 36′, the right being 93° 71′.

"2. The average temperature of the left parietal station is 94° 44′, the right being 93° 59′.

"3. The average temperature of the left occipital station is 92° 66′, the right being 91° 94′.

"4. The average temperature of the left side of the head is 93° 83′, the right being 92° 92′.

"5. The average temperature of the whole head, exclusive of the vertex, is 93° 51′.

"6. The average temperature of the motor region of the vertex is 91° 67′.

"7. The average temperature of the whole head, inclusive of the vertex, is 92° 66′.

"8. If there be an alteration of temperature at any of the lateral stations of more than one and a half degrees above or below the average temperature of such station, this fact will justify a *suspicion* of abnormal change at that point.

"9. If there be an alteration of temperature at any of the lateral stations of more than two degrees above or below the average of such station, this fact will constitute *strong evidence* of the existence at this station of abnormal change.

"10. In proportion as the alteration of temperature at any individual station is increased or decreased beyond the figures just mentioned, in exact proportion will the strength of the evidence be increased, until, the maximum or minimum having been passed, the evidence will become almost conclusive.

"11. Should it so happen that such elevation of temperature above the average should be at any lateral stations on the right, causing a rise at this point beyond the average temperature at the corresponding station on the left, this would strengthen the suspicion or the evidence.

"12. These remarks apply with equal force to the average for the whole of either side, as well as to the average for the whole head.

"13. It is necessary to the validity of these conclusions that the contemporaneous bodily temperature should be normal, or that there should be a marked disproportion between it and the cerebral temperatures."

Dr. Gray detailed a case of glioma of the brain, which he had been able to locate during life at the side of the cerebrum, when the symptoms pointed to an affection of the base, and claims that it is the first case on record in which an intra-cranial morbid growth had been localized during life by a thermometer, and the diagnosis verified by a post-mortem. Dr. Gray spoke of other cases, which were, however, not followed by a post-mortem, and the publication of which he therefore reserves. He is now engaged upon the study of the pathological aspect of the question, but his material is not yet ready for publication.

REMARKS UPON DR. GRAY'S PAPER.

Dr. Webber.—This paper seems to be opening up a new line of investigation, especially the pathological part of it, and the results so far have been carefully worked out. Should further trial result as satisfactorily as in the case reported, this means of diagnosis will be of great practical importance, especially in doubtful cases. I had a case a month ago, the symptoms of which caused me to suspect tumor of the brain, just commencing, and I can imagine that a thermometrical observation, such as Dr. Gray has described, might be valuable in settling the diagnosis, should it be proven always to be reliable. It had seemed to me rather curious that the change in temperature should be so localized when the brain is not immediately connected with the skull or the scalp. Has Dr. Gray any explanation of that?

Dr. Gray.—I should think the thermometer would indicate largely the temperature of the cerebrum itself, seeing that it is only covered by the scalp and a thin layer of bone, the bone being thinnest at the three stations selected for observation.

Dr. Shaw.—There is one great source of error in the use of surface thermometers, namely that any pressure upon the bulb will cause the mercury to rise very perceptibly.

Dr. Gray.—I stated in my paper that I had a great deal of trouble from this source, but that it was possible to make a thermometer with a bulb three-eighths of an inch across which would obviate this source of error. A safe way is to shake the mercury down ten or twelve degrees below the normal temperature before taking an observation. This was the method which I adopted in my observations, and by so doing I consider that I eliminated the error from pressure upon the bulb.

Dr. Shaw.—I am of the same opinion as Dr. Webber. I can hardly see how we can have such localized temperatures outside.

Dr. Gray.—We can only arrive at conclusions, by exclusion. The superficial parts do not vary to the extent indicated by the thermometer, the variation then can only be due to the change in the temperature of the parts beneath.

Dr. Seguin.—It seems to me that Dr. Gray has largely obviated the error which has crept into former observations of this kind, by making it a rule *not* to consider the temperature as indicating a pathological condition of the brain, unless it is more than a degree and a half above or below the normal. I regret that Dr. Hammond is not present this evening, otherwise it would be interesting to go into a general discussion of cerebral thermometry. I think Dr. Gray has demonstrated the fact that delicate thermometers are not of great value in cerebral thermometry.

Dr. Gray.—I should have given Dr. Hammond credit for his observation had I known of it before I submitted my paper.

Dr. Beard.—I was not able to judge from the paper whether any observations were made upon the effect of emotional disturbances.

Dr. Gray.—My object has simply been to obtain something which would be of diagnostic value, as is the ophthalmoscope and the laryngoscope.

Dr. A. McL. Hamilton.—It has occurred to me that considerable deviation may arise from a functional condition of the scalp.

Dr. Seguin.—I would suggest the probable usefulness of a series of experiments upon animals, whereby the temperature of the scalp and that of the brain could be determined at the same time, the temperature of the brain being taken through perforations in the skull.

Dr. Webber.—The results thus obtained would be only approximately accurate, for exposure might mutilate the brain and produce an inflammatory condition of the scalp also, or a disturbance in the circulation of the scalp which would interfere.

Dr. Gray.—It seems to me, though it might gratify a curiosity to know what the actual temperature of the brain is, there would be nothing of practical importance gained, for, by making the observations in the way I have suggested, the same conditions are met with in every person, and though these observations are only relative, yet they are sufficiently accurate for diagnostic purposes. It would seem quite certain that morbid changes in the skull would cause a rise in temperature.

Dr. Webber.—The case reported by Dr. Gray is of great value, and it is to be hoped that many other cases will receive like attention. I have noticed in my own case, that, after mental labor the vertex is much hotter than the sides of my head, and almost always it is accompanied with a feeling of discomfort. I have looked upon this as a rise in the temperature of the scalp, but as to its origin, I do not know.

Dr. Kinnicut.—I was about to remark that the same condition exists in other parts of the body after mental labor, as for instance in the mouth a rise in temperature of seven-tenths of a degree, so that perhaps the condition observed in the scalp is only part and parcel of what takes place elsewhere.

Dr. Beard.—I have never been able to answer that question to myself satisfactorily. You get a rise of temperature not only after mental, but after physical exertion, and sometimes where there is no exertion at all. Sometimes I have thought it was brought about through the nerves. In the hand, and in other parts of the body, where there is no mental disturbance, there are variations in the temperature. When we look at a great orator in the midst of a speech, we notice an enlargement of the veins of the face, and the question arises, what relation has the cerebral circulation with the circulation we see.

Dr. Emerson.—The fact that there are local disturbances in the temperature of the body does not favor the theory which Dr. Gray has advanced.

There being no further discussion, Dr. F. P. Kinnicut, of New York, read the history of a case of

GENERAL DOUBLE ATHETOSIS.

E. D., æt. 14; the patient is of average size for his age. General physical development good. Intelligent countenance and apparently no marked mental impairment. Marked and typical athetosis of hands, arms, cervical and facial muscles. The head is constantly being turned from one side to the other, rather more frequently and to a greater degree to the right, but without any regularity as to direction. The athetoid movements of the facial muscles are *particularly* marked, causing strange grimaces on the patient's attempting to speak. The sterno-mastoid muscles of the two sides are equal in size, both apparently being somewhat hypertrophied. The movements of the fingers, hands and arms are not "jerky," they are vermicular, irregular, semi-tonic and exist to an extreme degree. Hypertrophy of the muscles of the hands is doubtful; there is apparently some of the muscles of the arms.

Inco-ordination of legs exists, but in a slight degree. The speech is affected, being "jerky," slow, apparently attended with much effort and causing a marked increase in the athetoid movements of all the affected parts.

By drawing the patient's attention, the movements are somewhat controlled. There is absolutely no paralysis and no disturbances of sensation.

The characteristics of double or general athetosis as distinguishing it from hemi-athetosis, would seem to be as follows:

1. An affection congenital in nature, or dating from earliest infancy; in other words a *primary* affection.
2. An athetosis bilateral in character and always involving the muscles of the neck and face.
3. The absence of all paralysis and of all sensory disturbances.
4. The existence of a greater or less degree of imbecility in the majority of the cases thus far observed.

REMARKS UPON DR. KINNICUT'S PAPER.

Dr. Hamilton.—Within the past few weeks I have seen, I think, an unique case of hemi-athetosis, the history of which was taken by the House Physician in the Epileptic and Paralytic Hospital. The peculiarity of the case is the entire absence of mental disease, and his age, thirteen years.

Dr. Seguin —I fail to see why the occurrence of athetosis is not a sign of brain disease. Disease of the cerebellum gives rise to disorders of locomotion without paralysis. Cases of sclerosis occur without any marked paralysis. I saw a man this morning in my office, who for several months has had tension of the muscles of the right leg and arm, and he has a tremor which is not present when he is quiet. In this case Prof. Westphal corroborated my diagnosis of sclerosis of the left hemisphere of the brain. I simply make these remarks in support of the view that choreic movements may result from brain disease.

Dr. Hamilton.—I think the fact remains, that in the majority of the cases of athetosis, they follow some brain disease, or are connected with some undeveloped condition of the brain, such as idiocy.

Dr. Seguin.—Still Dr. Hammond's original case was not preceded by paralysis.

Dr. Kinnicut.—It was preceded by epilepsy. An analysis of twenty-seven cases, recently made, gave only four cases in which there was not paralysis, two of which were Hammond's cases.

Dr. Webber.—I have myself, a short time since, seen a case of athetosis beginning in infancy, at the age of fourteen months, the first symptom of which was a convulsive attack. The patient told me that from that time until the present (she is now twenty-one years old), there has been a constant motion of the left hand when she is awake, unless interrupted by placing her hand in some position. I looked up this subject and found thirty-three cases, of which about the same number were affected upon the right side as upon the left side. In a large proportion of these cases, where the age was given, the disease began in infancy. In two cases, autopsy showed hemorrhage into the corpus striatum. There was softening around the hemorrhage. The locality of all these lesions was the lenticular nucleus, sometimes towards its posterior end, and the internal capsule was also affected. It is interesting to notice that Pitres has lately discovered white bundles extending through the internal capsule, irritation of which produces the same movements in the limbs as irritation of the cortex. The view which they take of it is, that the nerves of motion pass through the basis cerebri, and in the internal capsule are in bundles which are separate from one another. If this be so, we can see how irritation in one neighborhood may be communicated to other fibres, and thus keep up a slow motion. Charcot has, I believe, reported three autopsies where the lesion was in the optic thalamus. The cases of infantile origin, such as the one I have seen, such as Dr. Jewell reported last year, such as Dr. Seguin has referred to in the transactions of this Association, such as Dr. Berger, of Breslau, reports, in all of these cases there is a lesion of the brain substance, which I suppose is not yet fully demonstrated, but which seems to be somewhat of the nature of sclerosis. I had an opportunity of examining one case after death. It was not a case of athetosis, but one of those infantile cases followed by motor disorder. The part affected was the sphenoidal lobe around the inferior cornu of the lateral ventricle. The examination was not very satisfactory, but the gross appearance was more satisfactory than the microscopical examination. The specimen had been placed in alcohol. I suppose in those cases of infantile origin there is some such change as I have described. I doubt if there is always a hemorrhage. In other cases occurring in infancy, resulting in motor disturbances, I have found a slight tremor, which would connect these together, as a chain of cases of varying peculiarities which shade into each other, and which enable us to judge that the lesion is very much alike in all cases.

Dr. Kinnicut to Dr. Webber.—Do you regard the localization as more important than the lesion? Why is not that the all-important point?

Dr. Webber.—That is just what I meant to have said. I think there is probably an interstitial change which produces an irritation at that point. In the post-hemiplegic cases which I have seen, which are few, the motions were not at all alike. In none that I have seen, and in the reports which I have read, is there a resemblance to the disease we call chorea. The movements are involuntary, occurring without any effort of the will, and without any object in view. The movements in chorea are, I think, entirely different from those in post-hemiplegic cases, and the name of post-hemiplegic chorea should be discarded.

Dr. Seguin.—In my experience I have thought I have made out considerable distinction between the two conditions. The movements in the athetoid condition in the child are never jerking, but are movements of flexion and extension, almost rhythmically, almost vermiculâr. In my two cases of post-hemiplegic chorea there was nothing of the kind, the movements were jerking, and there were also certain actions which resembled the movements of ataxia; they were certainly remote from true athetosis. In my own mind I drew a plan of forms of spasm running all the way from chorea, and presenting a great many varieties, including ataxic movements, and looked upon those post-hemiplegic cases as more resembling chorea than any form of athetosis.

Dr. Webber.—I do not doubt that many cases of post-hemiplegic chorea have this character. I mean to say that cases can be found where they shade off into each other, and seem to belong to the same class. At one end of this series there is great motion, and again at the other end the movements run into those of athetosis.

Dr. Seguin.—Prof. Charcot is disposed to make the choreic group include all of this group, taking the same view of chorea as did Trousseau, making it into an immense family with genera. I would add that there is one radical difference between athetoid movements and chorea: In athetoid movements the fingers chiefly participate, while in chorea we get movements of the whole face, or the whole side of the body.

Dr. Kinnicut.—It would seem that only in this double form are the muscles of the extremities affected. In the bilateral form the muscles of the face have been affected.

There being no further remarks, Dr. V. P. Gibney, of New York, proceeded to read a paper entitled

CERVICAL PARAPLEGIA,

with a request that a diagnosis as to lesion be given by the Association. The following abstract contains the main points in the case.

A female child, aged eight years, was first seen June, 1876, at which time a plaster of Paris jacket and head-support was worn, having been first applied in January of the same year, the supposition being that she had caries of the cervical spine. The disease dated from October, 1874, at which time she had a strain by catching her head under the round of a chair. Nothing marked occurred from that time until she was placed under treatment, save an inability to rotate the head completely. When the plaster of Paris jacket was removed the opisthotonus was marked, and no deformity of the spine could be discovered, though other signs pointed to cervical caries. By Aug. 22d, of the same year, the opisthotonus was entirely relieved, and motion of the neck nearly, if not quite perfect, and the diagnosis of caries was abandoned. A month later there was cervical tenderness without deformity. Mechanical appliances were discontinned. A certain degree of stiffness remained from this time until February, 1878, without other marked signs, the child enjoying good health meantime. In February there was induration over the trapezii, and marked tenderness and pain upon any motion of the head. The above symptoms and signs increased, and the patient soon lost the use of the upper extremities. The deltoids became atrophied, and loss of flesh followed rapidly the loss of power in the lower extremities. Treatment was again begun with ergot and iodide of potassium, a drachm of the former three times daily, and eight grains of the latter four times daily, which was followed within a few days by rapid improvement, and in a week by apparently perfect recovery. The patient was removed from the hospital March 29th, in this condition. June 20th, 1878, patient was found with absolute loss of power in upper extremities, little or no anæsthesia, considerable power in lower extremities, former degree of torticollis, no pupillary disturbances, sphincters of bladder and rectum normal, no headache, no spinal deformity, but tenderness in the cervical and upper dorsal region; greatly emaciated, and suffering from bad hygiene.

REMARKS UPON DR. GIBNEY'S PAPER.

Dr. Emerson to Dr. Gibney.—How about reflex action?

Dr. Gibney.—Tickling the feet caused scarcely any reflex action.

Dr. Seguin.—If the case did not have a history of an injury I would be disposed to look upon it as a case of myelitis of the anterior horns. I think that is present; perhaps something else.

Dr. Webber.—Would not pressure upon the nerve explain the symptoms?

Dr. Emerson to Dr. Gibney.—Were there any eye symptoms?

Dr. Gibney.—No, sir.

Dr. Seguin.—Cervical pachymeningitis would be thought of, but that would not begin with neck symptoms. A lesion of the nerve roots would have to extend from the atlas to the seventh cervical vertebra. Has the phrenic nerve escaped?

Dr. Gibney.—Yes, sir.

Dr. Seguin.—Is the sterno-mastoid paralyzed? I would be disposed to think part of the trouble due to myelitis of the anterior horns.

Dr. Shaw.—It seems to me that pain upon exertion or motion does not look like myelitis.

Dr. Gibney.—I regret that I did not make a thorough ophthalmoscopic examination.

Dr. Seguin to Dr. Gibney.—Is the heart's action disturbed?

Dr. Gibney.—No, sir.

Dr. Emerson.—There is a case of a physician in this city who received a severe injury in the neck, below the third cervical vertebra, and as far down as the first or second dorsal vertebræ, by diving into shallow water. He was laid up, and for more than a year had pain upon rotation of the head. There were no eye symptoms, no retardation of the heart's action. By the use of counter-irritation over the spine the pain was entirely removed. There was no paralysis.

FRIDAY, JUNE 21ST—AFTERNOON SESSION.

The Association was called to order at 2.30 p. m., and upon motion, Dr. E. C. Spitzka, in the absence of President Webber, acted as presiding officer.

Dr. Seguin gave notice of an amendment to the constitution of the Association, whereby all matters of business shall be attended to by the Council, subject to the control of the Association.

Dr. J. J. Putnam, of Boston, proceeded at once to read his paper upon

THE INTRA-OCULAR CIRCULATION.

The paper consisted of a brief notice of some experiments by Dr. O. F. Wordsworth and Dr. J. J. Putnam, originally undertaken with a view of studying the effects of mechanical compression of the veins of the neck, upon the circulation within the eye, but bearing also upon the question of the cause or the venous pulse, the rhythmical activity of the vasomotor centres, and the action of nitrite of amyl.

The only general conclusion which Drs. Wordsworth and Putnam would draw from the results of their experiments, which are preliminary only, is that mechanical causes do not readily, if at all, induce an appreciable arterial congestion of the fundus oculi, and that the degree of venous fullness so produced is not such as to justify, if seen in an eye examined for the first time, the belief that it is pathological. They are led, therefore, to doubt whether such a condition as congestion of the retina unassociated with inflammation of greater or less degree, in which case the term congestion would be, properly speaking, a misnomer, is ever met with.

The authors of the above observations, while acknowledging the fewness of their experiments and their want of accord with those of other observers, still feel that their work was done with such care and precision as to incline them not to doubt their accuracy on account of the objection stated, and they are convinced, that in no department of science have errors of observation played a larger share than in the ophthalmoscopic study of the fundus oculi.

REMARKS UPON DR. PUTNAM'S PAPER.

Dr. Shaw.—Dr. Mathewson, of Brooklyn, examined my eye while I took nitrite of amyl. There was no change in the retina, and when I took quinine, there was no change in the retina or disk. We concluded that there was no dependence to be placed upon the appearance of the eye in cerebral anæmia and hyperæmia.

Dr. Gray.—The experiments to which Dr. Shaw alludes are valuable in certain respects, yet I hardly think they warrant the conclusions arrived at, for the first factor necessary to that argument remains unproven, namely, why is it that quinine produces hyperæmia or anæmia of the brain?

Dr. Seguin.—Unfortunately one of our members, who is himself much interested in this subject, is not with us this evening. As a matter of clinical experience I have arrived at the conclusion, that there is no great value to be placed upon this observation, partly because it is almost impossible to determine exactly between the normal and abnormal circulation, especially when only having seen the patient once. I have frequently seen cases where the retinas had previously been said to be congested, but in which I could find nothing that looked like hyperæmia. I am under the impression that the ophthalmoscope has been overrated in this respect. It would be very desirable, of course, if we could determine the difference between normal and abnormal vascular states within the eye.

Dr. Shaw.—It has always appeared to me a very difficult matter to determine a change in the intra-ocular circulation.

Dr. Putnam.—It is with great difficulty that changes in the circulation of the eye are brought about. There is an inelastic incasement, and it may be taken presumptively that changes in the circulation could not be readily seen. The blood enters the eye under peculiar circumstances, and spreads itself rapidly through the arterial tree, for the reason that the capsule does not stretch at all readily. I am sure Dr. Wordsworth considers it difficult indeed, when the disk is quite red, to say from one examination, that the condition observed is abnormal; the physiological limits allow of great variation.

Dr. Seguin.—In this relation, it might not be out of the way to call attention to the fact, that the circulations of the retina, and the nerve itself, are independent. It seems to me desirable to state, independently in our notice of cases, the appearance of the retina and the disk; to state whether the retinal vessels are straight, or twisted, or over full. In some accounts it is simply stated, that the fundus of the eye was congested—a very crude formula, in my opinion.

Dr. Gray.—It seems to me the thing to be proven is, that the circulation of the conjunctiva, brain and fundus are alike.

Dr. Spitzka.—There is one disease which is associated with congestive attacks, namely, paralysis of the insane, and that has been the subject of ophthalmoscopic investigation. Klein, of Vienna, and Clifford Allbutt, of England, agree in frequently finding lesions in the eye. In the patient whom I exhibited before the Association the other day, the veins are tortuous and rather broad, the arteries being of normal calibre.

Dr. Putnam.—I would like to ask if any observations have been made upon the condition of the large veins of the retina? If the large arteries are full, we would expect to see the veins full also. The question was, whether it is possible or easy to have arterial congestion where there was no irritation inside sufficient to cause an increased tension.

Dr. Shaw.—I am of the opinion that the case which Dr. Putnam has been speaking of does not come under the same head as those of paresis.

DR. BEARD'S VERBAL COMMUNICATION.

The discussion having closed, Dr. George M. Beard made brief remarks upon a case of agoraphobia occurring in a lady whom he had under his care. The peculiarity of this case was her inability to go to church, though previous to an attack of unpleasant head-symptoms while at church, she regularly attended church. The plan which Dr. Beard adopted for the relief of this symptom, was to build up and improve the general condition of the patient, after which she was able to attend church as before.

The second point which Dr. Beard brought out was in reference to the treatment of fibroid tumors by electricity. He thought it could be negatively proven, that the decrease in the size of the tumors, while under treatment with electricity, was brought about by an effect upon the nervous system.

Dr. Beard then made some remarks upon the "Jumpers, or jumping Frenchmen," who are to be found in the northern part of Maine, among the French Canadians and lumbermen of that region, and who have for a long time been known for jumping or striking on sudden excitation. It is a psychological and not a pathological state. It is not a nervous disease. It is the result of a psychical condition among a people of a low order of intellectual development. If one of these persons be suddenly told to strike another, he strikes right out, even if it be his own mother whom he hits. If he have an ax in his hand it makes no difference. If one be sitting by the edge of a pond of water and is struck suddenly, he will jump into the water, even at the risk of drowning. They will strike against a hot stove, or against a hard object. They seem to have no inhibitory power after sudden excitation. They are unable to speak English well, but if suddenly addressed in English, they respond automatically in English. It is a remarkable illustration of the involuntary life. Dr. Beard's attention was called to these people during the past year, and he has investigated the subject through various sources of evidence, but this summer he expects to do it more in detail in Maine. It is not an epidemic or a disease, but a fixed psychologic state, a survival of the habits of the middle ages. Very large numbers of these people are thus affected.

There being no remarks upon Dr. Beard's communication, Dr. Seguin read the next paper, which was by Dr. H. D. Schmidt, of New Orleans. The paper was illustrated by a box of microscopic specimens which accompanied it.

ON THE STRUCTURE AND FUNCTION OF THE GANGLIONIC BODIES OF THE CEREBRO-SPINAL AXIS.

To form a correct idea of the structure of the different nervous tissues, we must bear in mind that the whole nervous apparatus consists, in its central as well as its peripheral parts, of only three essentially nervous anatomical elements. These are fibrils, nuclei and granules, and, moreover, a certain intermediate so-called "inter-cellular" substance, by means of which granules lineally arranged in rows are united to each other and transformed into fibrils. The only difference existing between the individual nervous tissues consists in the morphological arrangement and mutual relations of these elements., The nerve medulla and tubular membrane of the dark-bordered nerve fibres, though they may serve some special purpose, must be regarded as non-essential to the liberation and propagation of nervous energy.

The axis cylinders of the dark-bordered nerve fibres are but the direct continuations of the subordinate branches of the processes of the ganglionic bodies. All axis cylinders consist of a number of individual nervous fibrils, which, united by a homogeneous inter-fibrillous substance, are surrounded by a delicate, tubular sheath. The individual fibrils of the axis cylinders are not *smooth* fibrils, but present a knotted appearance, which indicates their *original anatomical composition of elementary granules*, and can be recognized on all axis cylinders.

In passing from the axis cylinders towards the centre, we of course arrive at the processes of the ganglionic bodies ; and as the axis cylinders represent nothing else but the subordinate branches of the processes, it becomes obvious that both are very probably identical in structure. In the mutual relations of these elements, however, a slight difference is already observed, which consists in the individual fibrils not being applied to each other as closely in the processes as in the axis cylinders. Hence, in the processes the individuality and the granulous character of the fibrils becomes more apparent. Finally, upon the ganglionic body itself, the granules, of which the fibres are originally composed, are most distinct ;

but the individual fibrillæ cannot always be traced, for the longitudinal furrows have not proportionately increased in width. The best method of making preparations is that proposed by Gerlach.

From demonstrations which Dr. Schmidt gave, he drew the conclusion that the ganglionic body, or cell, is formed by a smaller or larger number of nervous fibrillæ, mostly derived from the axis cylinders of the nerve fibres, embracing an independent body, known as the nucleus. In embracing the nucleus, the fibrillæ of the different processes not only decussate, but probably also interlace with each other, particularly those in the immediate vicinity. While surrounding the nucleus, the fibrillæ form no direct communication with it, but remain isolated.

The nucleus of the ganglionic body differs from the nuclei of other tissues in the complexity of its construction. This complex structure was described in detail, and the statement made that all ganglionic bodies of the cerebro-spinal axis are constructed in the same manner. Dr. Schmidt gave a minute account of his researches in regard to the termination of the finest subdivisions of the processes in the grey matter, which were conducted, without regard to trouble or pains, for the purpose of coming to some definite conclusion. The results which he obtained from these examinations, made on specimens of grey matter of the spinal marrow, conscientiously prepared after Gerlach's own formula, were the same as he obtained from his examinations of uncolored chromic acid preparations, a number of years ago; and as these preparations were made with the greatest care and accuracy, and examined by superior objectives, Dr. Schmidt feels conscientiously bound to deny the existence of a fine nervous net-work in the grey matter of the spinal marrow, such as Gerlach has described, in which the finest ramifications of the processes of the ganglionic bodies terminate, and from which nerve fibres arise. As Gerlach's drawing of his net-work in Stricker's "Handbuch der Gewebelehre, etc." resembles, in some measure, the apparent net-work of the neuroglia, Dr. Schmidt is inclined to think that he mistook the latter for the former. The question now arises, in what manner do the ultimate ramifications of the processes in question terminate? Dr. Schmidt now suggests, as he has done once before, that most probably a communication may exist in the form of anastomoses, though he cannot adduce sufficient evidence to support this suggestion.

Dr. Schmidt stated that in all cases we meet with fibrillæ surrounding a nucleus, and in order to show that this is a rule in the construction of ganglionic bodies, he described the manner in which these bodies are formed in the cerebro-spinal axis of the *Amphiuma tridactylum*, already known for the enormous size of its colored blood corpuscles. From facts presented, he concluded that all ganglionic bodies are composed of two elements anatomically and functionally distinct from each other. The one which is represented by fine nervous fibrillæ undoubtedly serves to conduct nervous energy liberated by stimuli applied at the peripheral surface, or emanating from a centre. The function of the other element, the nucleus, is more difficult to determine, but is probably related to the liberation of potential energy residing in it. That the integrity of the tissue depends upon the integrity of its nuclei is a pathological fact. And it is for this reason that Dr. Schmidt ventures to direct the attention of neurologists to the condition of the nuclei of the ganglionic bodies in diseased nervous tissues constantly met with in organic diseases of the nervous system. He considers the ganglionic body as an organ destined for the generation and the distribution of nervous energy, but incapable of receiving sentient or conscious impressions, and states that it is obvious that impressions can only result in consciousness in those nervous elements which represent the terminus, as well as the beginning of the nervous apparatus in the cortical layer of the brain. He states, further, that to suppose the nucleus is the particular receiving organ, is not logical, for the reason that the nervous fibrillæ carrying the stimulus to the centres, do not terminate in the nucleus, but in a delicate net-work which extends throughout the whole cortical layer. It would appear more probable that the stimulus coming from the sentient surface of the periphery, after being increased in strength by an additional amount of liberated potential energy from the true nervous cell, viz., the nucleus of the ganglionic cell, passes along the nervous fibrillæ surrounding this body without interruption, and finally arrives in the terminal nervous net-work, to be, in proportion to its strength or intensity, diffused throughout its delicate meshes, and moreover to the granular substance itself.

Judging from the arrangement of the terminal elements in the grey matter of the spinal marrow, medulla oblongata, and other nerve centres, being analogous to that of those elements in the cortical layer of the brain, it seems reasonable to suppose that these parts also should be the seat of a limited amount of consciousness, which, though insufficient for intellectual manifestations, may suffice to give rise to those well-known reflex and automatic actions.

REMARKS UPON DR. SCHMIDT'S PAPER.

Dr. Spitzka.—I am agreeably surprised to find the results which I laid before the Association, yesterday, almost simultaneously confirmed by Dr. Schmidt. Although his researches were made on the amphiuma, and mine on the menobranchus, yet zoologically closely allied as these amphibians are, it was to be expected that both should present the same or similar histological peculiarities; and in a short preliminary communication in Dr. Jewell's journal, I have ventured to predicate the largely nucleated nerve cell, and the peculiar arrangement of the fibrils for the whole group of proteans and all amphibians with permanent external or internal gills.

In some points I must differ from Dr. Schmidt. He states that the amphiuma has numerous nerve cells scattered among the fibres of the white columns. From the analogous cases of the siren and menobranchus, I am inclined to believe that he has possibly mistaken free nuclei, analogous to the so-called free nuclei found scattered in the white substance of higher animals, for nerve cells. As regards Dr. Schmidt's views on the brain, when he states that the cortex is poor in cells, and seems to consider as the cortex the layer just outside of the thickly crowded free nuclei which border on the ventricular epithelium, I must again differ with him. In a series of preparations of hemispheric sections, beginning with the urodela and passing to the anoura, then the chelonia, and further on in the animal scale, one can trace a perfect gradation of the free nuclei zone into the cellular layer of the cortex. As the animal develops, the nerve fibres accumulate between the free nuclei and the epithelium, and as this mass grows it entirely separates the free nuclei (metamorphosing, as some of them do, into pyramidal cells) from the mother-epithelium. This relation is well seen in turtles. The thickening of the ventricular floor, which is the rudimentary trace of a corpus striatum, is very feebly marked in the menobranchus and siren; it is comparatively better developed in the salamander and the frog. In the lower amphibia its cells are perfectly continuous with the free nuclei, and become separated therefrom in higher animals by the development of white substance. Dr. Schmidt styles the columnar arranged cells around the central canal, nerve cells. I have found that the extremity turned towards the free lumen had an epithelial fringe, the other end was connected with fibrils, and have had positive proof of the correctness of Stilling's view, that the epithelia were in reality of a nervous nature, of which the turtle's optic lobes give good evidence.

There being no further remarks upon Dr. Schmidt's paper, Dr. E. C. Spitzka read the last paper of the session, entitled

THE SIGNIFICATION OF NEGATIVE PATHOLOGICAL EVIDENCE IN INSANITY.

The result of experimental and pathological researches having demonstrated the close relation between brain and mind, some investigators were induced to defend the dictum, that all insanity was due to palpable cerebral disease. Although this view was in part erroneous, it still constituted a great step in advance of the psychological abstractions which, until then, had usurped the field of mental pathology. It stimulated not only alienists, but also general pathologists, to devote more attention to a neglected field, and if it has done no other service, has accomplished this much, that more elaborate methods of diagnosis, and sounder principles of treatment, are gradually becoming adopted in the asylums of most civilized countries. Several of the hitherto imperfectly recognized forms of insanity have received a clear demarcation by being proven to be associated with constant and characteristic pathological processes.

The purely symptomatic forms, as acute mania and melancholia, are not accompanied by any histological change. Vascular anomalies are however present, and thus serve to reduce these forms also to a symptomatic basis. It must not be supposed, however, that anæmia is equivalent to melancholia, and hyperæmia to mania, but that the conditions must be associated with other impalpable, perhaps biochemical anomalies, while the vasomotor relaxation of the one, and the vasomotor hyperkineses of the other, are as little referable to any known somatic basis, as the vasomotor hyperæsthesia of epilepsy. In one form of melancholia, even the subtle element of biochemical change may be wanting, and purely dynamic factors be the basis; it is that complete atonicity which follows a strong sudden emotion, as fright or joy, and which is comparable to the anæsthesia of the retina which follows exposure to a strong light.

The view that all insanity was due to palpable cerebral disease, was soon abandoned by European investigators, in consequence of these and similar observations. Its revival by those in this country, who, from their official connection with asylums claim to be considered alienists, is based on such imperfect observation and crude logic, that equally with our transatlantic cotemporaries, we can afford to consider a serious consideration or refutation thereof superfluous.

Let us select, as a paradigma of those insanities which present neither vasomotor nor structural abnormalities, that intellectual form of chronic mania which the Germans term *Paranoia* or *Verrueckhteit.* A priori we need not look for any changes in a form of insanity whose main symptom is a delusion, analogous to normal (though perverted) thought in its component elements and logic. The delusions of the intellectual chronic maniac are widely different from the delusions of paresis and senile dementia.

A case was given, illustrating the fact to which Meynert alone has called special attention, that the cerebral structures may be completely healthy in this disease; the examination was thorough, specimens from every convolution preserved as quadruplicates immersed in different preservative media, and examined also in the fresh state, being carefully studied. All the structures, both of the cortex, ganglia and medullary tracts were found ideally perfect, as well as the tutamina cerebri. Not even that accumlation of granules in the adventitia, and pigmentation in the base of the pyramidal nerve cells, frequently found in sane subjects, was present, probably for the reason that the sane subjects examined had died at a more advanced age than this patient. According to some pathologists, these appearances would, if present, have been chronicled as lesions of insanity, with how little reason is obvious. In order to show that such changes, especially in that extreme degree which is found with senility, have no real relation to the symptoms of chronic mania, the speaker related another case of a chronic maniac who had died at the advanced age of eighty, in whom moderate but general pigmentation of the cortical cells, and intense pigmentation of the nerve cells of the medulla, besides several small cysts, were found. These appearances are accompaniments of the senile state, as well in the previously sane as in the insane, and they affect previously contracted delusions, just as they weaken normal ideas, rendering them less original and energetic.

From these and several similar negative observations, we could learn the following lessons: First. That the changes to which the sane brain is subject should be carefully studied in order that we may be better able to exclude the possibility of their bearing a specific relation to insanity. Having done this, we should enquire whether a given change, which qualitatively does not necessarily cause insanity, may not produce alienation quantitatively. Second. That not every lesion found in advanced insanity stands in a causal and determining relation thereto; aside from the element of accident, we must bear in mind that such lesion may be the result of fluxionary hyperæmias oft repeated (as in querulant delusions and recurrent mania) as well as of the original pathological process with which the insanity may have started. For example, to infer that because one or two paretics out of a hundred have miliary aneurisms, miliary aneurisms are the lesion determining the symptoms of paresis, would be as absurd as to conclude that the extreme degree of cerebral congestive hyperæmia found in a patient who dies comatose and delirious from being confined in an Utica crib, was the cause of his previous, often very harmless, insanity. The third lesson taught is, that debarred from finding the pathogenesis of all forms of insanity on a purely histological basis, we must look to other solutions of the problem. Although the chemical changes occurring in the brain at the moment of death are presumably so much more intense than the delicate biochemical modifications which, compatible with life produced functional under or over-excitation, and may, therefore, blot them out almost entirely, yet in extreme cases something may still be elucidated.

A fourth lesson is, that in endeavoring to obtain an insight into the mechanism of acute mental symptoms, we can only take into account the appearances found in the bodies of those who presented these symptoms immediately before death. If a patient who has suffered for several months from melancholia, dies in the delirium of a fever, we cannot expect to find anything in his brain pointing to the causation of the symptom melancholia, but rather to delirium.

Still another lesson is, that inasmuch as in intellectual insanity the abnormal mental processes though perverted are yet analogous to normal mental processes, we shall look to neither a morbid histological, vasomotor, or biochemical change. In these cases, asymmetries of the hemispheres, incongruities of development in the peduncular tracts, heterotopia and disproportionate development of the centres on one and the same side, have been found by Muhr, Betz, Zenker and myself. Although found in but a few cases, yet the temptation to judge from the condition found in them, to less palpable but similar ones, in all or at least most cases of chronic mania, is a great one, when we recollect how pre-eminently hereditary this disease is, and how logically correct it is to seek for incongruities in the anatomical associations, as the basis of faulty psycho-physiological action.

Before closing, I would refer to an error which has crept into the etiology of reflex insanity. Many alienists would unquestionably style a patient who, having developed a delusion, that he has the devil in his belly, is found to have extensive adhesions binding down the intestines, a case of reflex insanity. Now as at least in one of these cases I have been able to trace the insanity further back than the peritonitis, and the subjective abdominal sensation had obviously only that accidental relation to the patient's insanity which every external circumstance, even outside the patient's body, might have in causing special delusions.

However, with all the negative pathological evidences in insanity, we find only confirmations of the conclusion that all abnormal, like all normal cerebration, has its immediate seat in the brain.

The true import of this negative evidence is to point out that the search for the somatic basis of insanity should not be a one-sided one, and that a rational psycho-pathology cannot afford to neglect any of the methods offered by anatomy, histology, chemistry, embryology or physics. The further we will be able to carry our reasoning by exclusion even, the more distinctly will the proper lines of research to be followed, be appreciated.

There being no remarks upon the above paper, and no further business, the Association adjourned, to meet in New York on the third Wednesday in June, 1879.